MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXX. *December* 10, 1869. No. 2.

ADMIRAL MANNERS, President, in the Chair.

Charles Joseph Corbett, Esq., Imber Court, Thames Ditton;
Charles Lambert, Esq., Queen Street Place, Thames Street;
and
John Wood, Esq., Wharf College, near Tadcaster,

were balloted for and duly elected Fellows of the Society.

The November Meteors, 1869, observed at Port Said.

By G. L. Tupman, Esq.

On the morning of the 8th November, while observing shooting stars, I suspected radiation from the constellation of *Leo*. The following night was too cloudy for any observations whatever to be made.

On the 9th, between $12^h\ 45^m$ and $15^h\ 45^m$, radiation from *Leo* was observed at the rate of 8 per hour, principally small meteors. At the same time the radiation from two radiants in *Taurus* was at the rate of 12 per hour.

On the 10th, between 13^h and 16^h, the radiation from *Leo* was at the rate of about 6 per hour, and from *Taurus* about 7 per hour.

On the 11th, from $12^h\ 50^m$ to $15^h\ 10^m$, there was no radiation whatever from *Leo*, or indeed from any other point, although during that time shooting stars were observed at the rate of 16 per hour.

I am, as yet, unable to determine the radiant point in *Leo*, on the 9th and 10th, so that it is impossible to say if the meteors mentioned belonged to the true November system or not. Probably not.

On the 12th, out of thirteen observed meteors, four certainly radiated from near *γ Leonis*. One at $14^h\ 50^m$ passed exactly over the stars *μ* and *α Ursæ Majoris*, leaving a dazzling streak midway between those stars. This number reduced to the zenith would give about 5 per hour for a single observer.

On the 13th, a very strong breeze was blowing from the northward, accompanied by heavy clouds, which occasionally nearly altogether obscured the heavens. Above the lower cloud stratum was a thick haze which prevented the smaller stars being seen.

From $12^h\ 30^m$ to $13^h\ 15^m$, a pretty large patch being clear overhead, two meteors only were seen, neither of which radiated from *Leo*. It then became overcast, and I felt convinced that the shower was either all over or had not yet commenced.

The watch was resumed at $14^h\ 30^m$, the sky being then partly clear in patches, and continued until a quarter past 5, long before which the shower had entirely ceased. At $2^h\ 30^m$ it was at its height, most of the meteors being remarkably brilliant, and many of them tinted green. The greater part left bright streaks, which often remained visible a considerable time. The duration of the meteors or their "time of flight" was considered to be less than half a second—too short a time to estimate even roughly.

The following are the observations. Being unassisted, I stopped at every sixteenth to make the necessary entries:—

13 November, 1869, Alexandria Mean Time.

From	To	No. of Meteors.	Elevation of the Radiant.
h m	h m		°
10 40·0	13 15·0	0	15
14 30·0	14 40·0	16	35
14 52·0	15 2·5	16	40
15 8·0	15 19·7	16	43
15 24·0	15 33·6	16	46
15 38·5	15 52·5	16	50
15 59·0	16 7·4	16	54
16 12·0	16 24·0	16	57
16 26·0	16 38·0	6*	60
16 40·0	16 52·0	7	63
16 54·0	17 14·0	4	67

Seven other meteors were observed, but they did not radiate from *Leo*.

If the numbers in the above table be reduced to an uniform

* During this observation it was more cloudy than before, but during the two following ones it was much clearer.

interval of time and then multiplied by the cosecant of the altitude of the radiant, it will be seen that between $14^h\ 30^m$ and $16^h\ 24^m$ the numbers were nearly uniform, and slightly decreasing. The maximum, then, was either before or about $14^h 30^m$; but the *centre* of the dense part must have been passed about 15 hours, as there was no sign of the shower at $13^h\ 15^m$.

For the determination of the Radiant point, eleven orbits were marked off on the chart, which appeared so close to the radiant as to give its position much more accurately than a host of orbits further off. The following table is a list of these orbits. The right ascensions and declinations are measured from the equinox of 1830, and, consequently, the resulting radiant point requires correcting for 39 years precession. Those of the first order of merit are more valuable than the others :

No.	Appearance.		Disappearance.		Order of Merit.
	α	δ	α	δ	
	°	°	°	°	
1	151½	+30	152	+42	2
2	154½	+21¼	158	+21¼	1
3	161	+20¾	172	+19¾	2
4	152½	+19¼	157	+16¾	2
5	150	+19½	149¼	+14	1
6	149¼	+19	147½	+14½	1
7	140	0	144	−12	2
8	130	+7	117	−3	2
9	112	+5½	84	−8½	1
10	143	+19½	136	+16	2
11	140	+19½	127	+17½	2

These give the Radiant $\alpha = 151°{\cdot}0$, $\delta = 21°{\cdot}5$, measured from the equinox of 1869. But it must be noticed that no single point will satisfy all the paths that were observed, which proves that their orbits cannot be identical in inclination and eccentricity.

Three of the meteors deserve special notice.

The first occurred at $15^h\ 32^m$ near θ *Hydræ*. After describing an arc of 10° or 12°, almost instantaneously it exploded with a blaze of light that illuminated the whole sky, and was mistaken for lightning. At the point of explosion, a small luminous cloud was formed, about a degree and a half in diameter, of the shape of a nearly closed horseshoe, the interior diameter being about one-third the exterior. Its whole light was about equal to that of a first magnitude star, or of the nebula in *Lyræ*, seen in a large telescope. During the two or three minutes that attention was paid to it, it did not sensibly diminish its lustre or alter its place, which was $\alpha = 141°$, $\delta = +4°$.

The second occurred at $15^h\ 40^m$ in *Ursa Major*. It lighted up everything around as a brilliant flash of green lightning, and left an exceedingly bright streak some 8° in length, the centre of

which was in $\alpha = 131°$, $\delta = +55°$. After about a minute, this streak assumed a beautiful wavy appearance ~~~~, and two or three minutes after one of the shape of an S, the axis of which was inclined to the original axis about 70°. Five minutes after its first appearance it had drifted over the star *o Ursæ*, and was still pretty bright when it was obscured by clouds.

The third was very bright, of a greenish hue, and exploded like the first, but behind thin cloud, which did not prevent its illuminating all around. Unfortunately, its position could not afterwards be identified.

On the 14th, from $14^h\ 30^m$ to $15^h\ 20^m$, about a third of the sky being very clear, five small shooting stars only were seen, radiating from *Orion*.

The thickness of the dense part of the stream must have been about 52,000 miles, measured perpendicularly to the plane of the orbit. It may only have been one aggregation among many, and may not have been situated centrally in the stream.

From these observations the elements of the orbit are

$$\pi = 62^\circ\ 36'$$
$$I = 15\ 38$$
$$\Omega = 231\ 44$$
$$\varepsilon = \cdot 9062$$

Motion retrograde.

Assuming a periodic time of 33¼ years.

Port Saïd, Lower Egypt,
16 November, 1869.

The Assistant-Secretary, Mr. Williams, read some extracts from the introductory remarks to the MS. work presented to the Society by him at the last meeting, being a translation of the accounts of comets observed in China from B.C. 613 to A.D. 1640, with the original text, of which introductory remarks the following is a brief abstract:—

According to Chinese tradition the Emperor Shin Nung, the successor of Fuh He, the founder of the empire, was the first who instituted astronomical observations. His reign commenced B.C. 3218. One of his successors, Hwang Te, is reputed to have been the monarch who established the mode of reckoning their chronology by cycles of 60 years, in use to the present day. The first of these cycles commenced B.C. 2637. He is also said to have discovered the lunar cycle of 19 years, by which the intercalary moons were to be regulated, from which it should appear that this cycle was known to the Chinese about 2000 years before it was introduced into Greek astronomy by Meton. These, however, must be looked upon merely as Chinese traditions, having no other authority.

In the most ancient of the Chinese historical works, the Shoo King, the instructions of the Emperor Yaou, who ascended the throne B.C. 2356, to his astronomers He and Ho are mentioned. In these they are, first, commanded to record their observations of the heavenly bodies, to complete the calendar, to make an instrument to show the motions of the Sun, Moon, and stars, and with due respect to impart information respecting the seasons to the people. In the succeeding four paragraphs four astronomers are sent to places in the east, south, west, and north, to observe the equinoxes and solstices, and certain stars are named as indicating the seasons. The first of these stars answers to our *α Hydræ*, which is described as culminating at sunset on the day of the vernal equinox. From this it seems that the vernal equinoctial point was then in the *Pleiades*, and consequently by the precession of the equinoxes it would appear that this observation was made B.C. 2306, being the 56th year of the reign of Yaou, thus affording a strong presumptive proof of the veracity of Chinese history as recorded in the Shoo King. The remaining three paragraphs relate to the labours of the other three observers; and, in conclusion, Yaou calls the attention of the astronomers to the fact that the year consists of 366 days, and among other matters of less importance to astronomy directs them to fix the intercalary moons, by which the seasons were to be kept in their places. Many other works are mentioned, both ancient and modern, as containing astronomical notices, and among these the great historical work called the *She Ke* and the Encyclopædia of Ma Twan Lin, are more particularly noticed as being those from which the greater number of the observations of comets contained in this volume have been extracted. The number of these observations thus brought together amounts to 370, a few of these may be meteors, but the far greater number are undoubtedly comets. Those mentioned by E. Biot in his communication to the *Connaissance des Temps* for 1846, amount to 224, consequently, in the present catalogue there are about 146 more than are given in that work; and there is reason to believe that the list now under consideration is as complete as any that has hitherto appeared.

The observations taken singly may be considered as divided into two parts, the one chronological, and the other astronomical. For reducing the first to our reckoning, certain tables have been constructed consisting of a complete set of chronological tables to be used in finding the years, and of others needed in reducing the moons and days. The construction and use of these tables are fully explained, and illustrated by examples of their application, and by their aid any year, moon, or day mentioned in the Chinese records can readily be reduced to our time.

For the astronomical portion, a complete celestial atlas, traced from the cuts in a Chinese work on astronomy, has been made, and which, as well as the preceding tables, forms a part of the work. In this the twenty-eight stellar divisions and the three

great spaces in which the stars are distributed by the Chinese are figured and described. To this atlas there is a full explanation, so that any group of stars mentioned in Chinese astronomy may be readily found by means of the index to the names; and the accordance of these groups with our constellations, is also made clear by means of reduced copies of the figures in Flamsteed's Atlas, with the Chinese asterisms laid down on the corresponding stars. There are many other interesting particulars mentioned in these introductory remarks which time did not allow to be entered upon, particularly an enumeration of the subjects treated of in the sections and chapters of the astronomical division of the history of the Ming dynasty 1368–1644, which, among other matters of interest, contain not only cometary observations, but also a catalogue of stars, with their longitudes and latitudes, both on the equator and the ecliptic, and more than seventy pages of observations of occultations of stars by the Moon and the planets.

Mr. Williams concluded by expressing his conviction that in placing this volume in the library of the Society he had secured it a position in which it is the most likely to be of service in future investigations into the subject of Chinese astronomy.

On Auroral Appearances and their Connexion with the Phenomena of Terrestrial Magnetism. By Balfour Stewart, F.R.A.S.

Some years since I ventured to suggest that auroral displays might be secondary currents due to small but rapid changes, caused by some unknown influence in the magnetism of the Earth. In developing this idea, the Earth was compared to the core of a Ruhmkorff machine, and the moist upper strata of the Earth, as well as the upper strata of the atmosphere, to secondary conductors, in which currents will take place whenever the magnetism of the Earth changes from any cause. These views would appear to be confirmed by the very interesting records of earth-currents obtained by Mr. Airy at the Greenwich Observatory, in which it is found that during times of great magnetic disturbance there are strong earth-currents alternating from positive to negative, the curves lying nearly equally on both sides of the zero.

A further development of this idea has lately occurred to me, in consequence of a remark of my friend Mr. Lockyer, that the zodiacal light may possibly be a terrestrial phenomenon, and may therefore be somehow connected with the phenomena of terrestrial magnetism. For not only will secondary currents be caused in a stationary conductor in presence of a magnetic core of variable power, but also in a conductor moving across the lines of force of a constant magnet. The question arises, have we on the Earth such moving conductors? In answer to this, let us reflect what takes place at the equator. When once the anti-trades have reached the upper regions of the atmosphere,

they will become conductors from their tenuity ; and as they pass rapidly over the lines of the Earth's magnetic force we may expect them to be the vehicles of an electric current, and possibly to be lit up as attenuated gases are when they conduct electricity. May not these form the Zodiacal light ?

Such moving currents will of course re-act on the magnetism of the Earth. We may therefore suppose that somewhat sudden and violent changes are likely to take place in the Earth's magnetism at those seasons at which the Earth's great wind-currents change most rapidly. May not this account for the excess of disturbances at the equinoxes ?

Besides the anti-trades there are also no doubt convection currents caused by the daily progress of the Sun taking place in the upper regions of the Earth's atmosphere. May not these also be the vehicle of currents as they cross the lines of the Earth's force, and account, to some extent at least, for the daily variations of terrestrial magnetism ? and may not this be the reason of the likeness observed by Mr. Baxendell between the curves denoting the daily progress of the wind and those denoting the variation of the declination magnet? Such currents, in as far as they are electric conductors, taking place in the upper regions of the atmosphere would not be felt by the earth-current wires at Greenwich, and I think Mr. Airy has noticed that this is the case. But the tidal wave represents a motion of a conductor on the Earth's surface, with two periods in one lunar day. This motion cannot produce a very great secondary current, but may it not be sufficient to account for the lunar-diurnal magnetic variation, which is also very small ?

Such a current taking place in a conductor electrically connected with the Earth's upper surface ought to be felt by the Greenwich wires, and, if I am not mistaken, Mr. Airy has detected a current of this nature.

May we not also imagine that there are two varieties of Aurora, one corresponding to stationary conductors under a very rapidly changing core, and the other to rapidly moving conductors under a constant core ? And might not an Aurora of the latter kind indicate the approach of a change of weather ?

These remarks are thrown out in order to invite comment and criticism, and they will have served their purpose if they direct attention to the part that may be played by moving conductors in the phenomena of terrestrial magnetism. It will be noticed that these remarks do not touch upon the mysterious and interesting connexion believed to exist between magnetic disturbances and the frequency of solar spots.

P.S.—Since writing the above, Sir W. Thomson has called my attention to a paper by him in the *Philosophical Magazine* for December 1851, in which it is suggested that moving conductors may play a part in the phenomena of terrestrial magnetism.

Note on Mr. De La Rue's paper "On some attempts to render the luminous prominences visible without the use of the Spectroscope." By William Huggins, Esq.

Mr. De La Rue puts the following question, "The proposal of Huggins to combine the employment of the spectroscope with the use of absorbing media naturally suggests the question,—why use the spectroscope at all?"

The suggestion contained in this question was one of the methods by which I originally attempted to render the red flames visible. Some three or four years since, almost at the same time that the method of reducing the scattered light of our atmosphere by the use of dispersion occurred to me, the method by absorption also presented itself to me as a very feasible plan. I made many attempts, not only with the spectroscope, but also with variously combined coloured media without the spectroscope. The latter plan, namely, by absorption, appeared to me to be peculiarly desirable, if it could be accomplished, as it would enable the observer to see the forms of the prominences, and also the whole disk of the Sun at once. It could be applied to any telescope, or used with an opera-glass, or even with the naked eye.

I referred to this method in the report of my Observatory, *Monthly Notices*, vol. xxviii. p. 88. Subsequently, when the Indian observations had confirmed my suspicion that the prominences would give bright lines, and also shown their position in the spectrum, I gave in a short Note an account of this possible method of viewing the solar prominences: (*Monthly Notices*, vol. xxix. p. 4.) The method is also described in my paper in the *Phil. Trans.* 1868, p. 551.

This method was also independently suggested by Lieut. John Herschel in September, 1868: (*Monthly Notices*, vol. xxix. p. 5.)

During the early part of the present year I tried a large number of coloured media. The difficulty is to combine two media which shall absorb light of all refrangibilities except precisely that of the line C or the line F. If even a small range of refrangibility besides that of the line selected, say C, be allowed to pass, the scattered light of the atmosphere overpowers and eclipses the prominences.

The most promising media of those which I tried, were a solution of carmine in ammonia, which cuts off very nearly all the light more refrangible than C, combined with a solution of chlorophyll, which gives a strong band of absorption, taking away the brighter part of the light less refrangible than C. Unfortunately, however, the chlorophyll band encroaches a little upon C, and so weakens the light of the prominences. The absorption of chlorophyll, as Prof. Stokes has shown, can be moved a little in the spectrum by acids and alkalies, and differs slightly in different plants; but I have not been able to degrade the band sufficiently to allow light of the refrangibility of C to pass wholly unimpeded.

The proposal stated by Mr. De La Rue of combining the employment of the spectroscope with the use of absorbing media, refers probably to a method I proposed for rendering the *forms* of the prominences visible in the spectroscope. In a Note to the Royal Society, *Proceedings*, vol. xvii. p. 302, I showed that by using a wide slit, the forms of the prominences could be seen directly ; and I then proposed using a deep red glass before the eye-piece of the spectroscope for the purpose of relieving the eye from the glare, which, under some circumstances, is painful, when a wide slit is employed.

On Spectroscopic Observations of the Transit of Venus in 1874.

By R. A. Proctor, B.A.

At the last Meeting of this Society it was mentioned that the American observers had suggested that spectroscopy should be applied to the observation of the approaching transits of *Venus*. It had been noticed during the total eclipse of last August that the approach of the Moon to the Sun's disk was rendered sensible before the actual commencement of the eclipse by the obliteration of the bright lines of the chromosphere ; and the American astronomers suggest that the same might be the case when *Venus* is approaching the Sun's disk in 1874.

I think Mr. Huggins gave a practical character to this suggestion by pointing out that, although there would be difficulties in the method, applied merely to determine the motion of *Venus* over the chromosphere, yet that, as giving a means of preparing the observer for the occurrence of external contact, the method would perhaps be valuable. It is clear that this is, in fact, the only sort of observation which would have any value, since the chromosphere is not an envelope of uniform depth, and *Venus*, as seen from different parts of the Earth's surface, would appear to cross different parts of the chromosphere.

There is a further objection which I pointed out at the last Meeting in the fact that *Venus* approaches the Sun's limb not at a right, but at an exceedingly acute, angle, and that, as we do not know certainly the point of contact, we cannot tell the angle at which *Venus* will cross the limb.

Further, the stations best suited for observing internal contacts are not all suited for the particular mode of observation proposed by the American astronomers. At Crozet Island, for example, where internal contact most retarded can be most favourably seen, the Sun will be close to the horizon when *Venus* is traversing the chromosphere.

Passing over this last objection, which applies equally to egress and to ingress, it will be clear that Mr. Huggins' excellent suggestion that the passage of *Venus* over the chromosphere can be made the means of determining with extreme exactitude, the moment of external contact, can be applied satisfactorily to ob-

servations of *Venus* in egress, since the observer can follow the planet across the Sun's limb with perfect certainty as to the point of last contact.

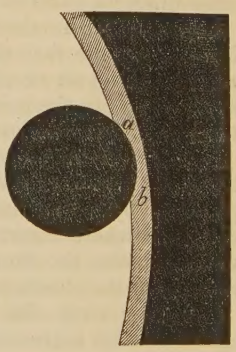
It has occurred to me that the method of observing the prominences by means of an open slit (high dispersive power being used) might be adopted with advantage for determining the moments not only of external, but of internal contacts.

It is clear that so far as external contacts are concerned, any method which rendered the prominences and the chromosphere visible would serve to exhibit the passage of *Venus* over these objects, and so to indicate with extreme exactitude her first contact with the Sun's limb. I would invite particular attention to the extreme slowness with which, in 1874, *Venus* crosses the limb of the Sun. In a paper which appeared in the Supplementary Number of the *Monthly Notices*, I called attention to certain effects of this peculiarity. According to ordinary modes of observation these effects would be disadvantageous, though they are compensated by other advantages. But, according to the suggested mode of observation, the slowness with which *Venus* approaches the Sun's limb would be a great advantage. If we assign to the chromosphere an apparent height of about 10″, *Venus* will occupy about $4\frac{1}{2}$ minutes in crossing that envelope, and this would afford the observer ample time to search for her. At egress, of course, it would be quite easy to follow the motion of her disk across the chromosphere.

In this plan the direction of the open slit would be at right angles to the Sun's limb; and it will be very obvious that internal contacts could be watched in the same way, and, perhaps, with more exactitude than by the ordinary method.

But it has occurred to me that internal contacts might be observed with advantage in a different manner, and probably Mr. Huggins had some similar plan in his thoughts, when at the last Meeting he suggested the use of a tangential slit.

If the open slit were placed tangentially, so as to include the solar cusps *a* and *b*, it is very clear that there would be seen two solar spectra separated by a dark space across which could lie the image of the chromosphere, which, however, would not be visible, owing to the brightness of the solar spectra. As *Venus* gradually moved from the Sun's limb the two solar spectra, or at least their inner edges, would appear to approach, and at the moment of real internal contact the dark space between them would disappear.



I think the moment of internal contact might thus be determined much more exactly than by direct observation. Of course, the method in reality amounts to changing the points of the cusps into parallel

lines and determining the moment at which these lines become coincident. But it seems obvious that, so far at least as optical difficulties are concerned, an observation of this sort would be much more easily made than an observation of "the breaking or formation of the dark ligament."

This method is clearly applicable to egress also, and applies as well to Halley's as to Delisle's method. The best stations for applying it are those referred to in my paper in the *Monthly Notices* for last June.

Nov. 22, 1869.

On a Change in the Colour of the Equatorial Belt of Jupiter.

By John Browning.

For several years I have been in the habit of observing *Jupiter* with considerable regularity with reflectors of large aperture. On every occasion previous to this presentation, the equatorial cloud-belt has been without colour, and the brightest portion of the disk of the planet. During the month of October this cloud-belt has, however, constantly been of a strong, greenish yellow, and darker than the bright belts north and south of it. The colour is almost exactly that known to artists as yellow lake. The colours of various parts of the disk I take from my observatory note-book.

Oct. 9th, 9·30.—Equatorial belt strong yellow lake, the other bright belts colourless. Dark belts very warm grey, and faint coppery red. The poles of the planet ashy blue. The drawing I have the honour of exhibiting represents the planet as it appeared at this time.

Two or three persons quite unused to the telescope noticed the colour of the equatorial belt.

Oct. 11th.—Observed *Jupiter* with Mr. Proctor. Colours the same, but the greenish-yellow belt was covered with white spots. Mr. Proctor agreed with me as to the greenish yellow of the equatorial belt and the ashy blue of the poles, but could not see the red in the dark belts.

In all instances I used 12 inches aperture and a power of 148, *achromatic* eye-piece.

I have received a letter from Mr. Brindley, of Lewisham, from which I give an extract:—"With reference to the appearance of *Jupiter*, I last night spent some hours with the $8\frac{1}{2}$-inch telescope turned on him. I was most agreeably surprised; the belts were so different to what I had seen them before. The dark ones of a dark lake colour, the bright one of a lovely tinted green." As this letter was written some days after my drawing was made, and the writer had not seen the drawing, I cannot be much mistaken in the colours I have ascribed to the various portions of the planet.

Two years since I saw a coloured drawing of *Jupiter* at Mr. De La Rue's Observatory at Cranford. In this drawing, if my memory serves me, the equatorial belt was colourless.

I have watched the planet through the greater part of a revolution; the greenish yellow of the great equatorial belt remained unchanged. I have drawn the attention of several observers to the colours of the belt, some of whom have been in the habit of observing it for many years. One of these gentlemen says that he is certain that the equatorial belt has not presented such an appearance as at present for a quarter of a century.*

Writing to Mr. Proctor some time since, I said that I thought the very small specific gravity of *Jupiter* might be due to the existence of a cloudy envelope of enormous extent. Mr. Proctor replied, that he thought it just possible that the whole light from the planet might not be reflected; but that some portion might be emitted by the body of the planet. The same idea had presented itself to me. Being by far the largest planet in the solar system, it will certainly have retained more heat than the rest.

Such an alteration in the colour of the planet as I have described, must, I think, indicate some considerable change, either on the surface of the planet or in its atmosphere. In the hope of throwing some light on such changes in future, I am making a careful map of the spectrum of the planet. When this is completed I shall have the honour of submitting it to the Society.

Selenographical Notes: Apenninus and adjacent Regions.

By C. H. Weston, Esq.

Few lunar objects are more striking than the range of *Apenninus* lying S.E. of *Mare Imbrium*. It not only presents forms unlike the commonly existing circular structures, but when viewed on the *terminator* of an increasing Moon (and definition good) exhibits broad shadings on its massive flanks and serrated outlines mapped out in light upon the low lands far beneath.†

During the last month the decreasing Moon was examined under favourable circumstances on the morning of the 27th when just east of the meridian. Her age about $20^{d}\ 13^{h}\ 30^{m}$. By her libration a little more of the NNE. quadrant was made visible than in mean position. The *terminator* was particularly interesting, as its extremities passed so very near both the lunar poles. On or near the *terminator* the following striking points were carefully identified:—Region near North Pole, *Anaxagoras* on east of terminator, *Barrow A* on west (shadow fine), *Epigenes* on east, *Aristoteles*, *Eudoxus*, *Calippus*, on east, *Menelaus* on east, *Agrippa* on east, *Godin* on east, *Delambre*, *Taylor* on east, south-west end of valley between *Taylor* and b.c. south-east of *Taylor*, *Kant*, *Descartes*, *Abulfeda*, *Almanon*, *Sacro-bosco*,

* Since writing the above I have received information which leads me to suspect periodicity in the change of colour on *Jupiter*.

† During a partial eclipse (when illumined objects could be contrasted with dark parts) details were finely seen by the larger Newtonian (14·25 in. 16 ft.)

Gemma Frisius, *Maurolycus*, *Clairaut*, *Jacobi*, *Pentland*, *Simpelius*, and region north-west of South Pole. On the south-east *Sharp* could be detected, and the high mountain range north-west of *Newton*.* The extreme south peripheral regions of mean Moon were cut off by libration.†

On 26th *Scoresby* (south-west of North Pole) and two contiguous *ellipses* were seen, but (by the same libration) under greater than the mean visual angles, and therefore apparently more circular than in Beer and Mädler's map. *Gioja*, too, and the mountainous range still further north, which conceals the North Pole point.‡

It is best, first, to describe the cliff-like coast line of *Apenninus* and adjacent regions areographically. The lower general surface of *Mare Imbrium* (except when broken by the rays of *Copernicus* and low ranges) is apparently continued at about the same level to *Apenninus*, and extends from *Eratosthenes* to the vicinity of *Huygens*, where, I conceive, should have been introduced in the valuable lunar map an equally dark continuous shading, as far as and around *Huygens*, to meet the shading at the foot of the promontory in the rear, and so sweeping round and onward to the ranges still more retired. At the last point begins the elevation of the floor of *Mare Imbrium*, and extends north-east towards *Timocharis* and round to *Archimedes*. I do not think, however, that this elevation should have been continued unbroken or so little broken. There seems to be an interrupted valley of some width, running north-east and south-west from *Archimedes* to *Apenninus*, co-extensive with the base of the former, and somewhat, perhaps, wider near the latter. Under low telescopic powers (in this phase of the Moon) it assumes not only a tripartite structure, but (at the end abutting against *Apenninus*) a broken shaded triangular form. Under high powers we perceive in it incipient or low protrusions, circular and rugged, and the shaded triangular part loses its character and is found marked with several hill ranges and deep dark combes. The *south-west patch* has considerable ridges, consisting apparently of great arcs of curves, connected like festoons with the convexities towards the valley, and together occupying about its whole length. It is true, indeed, that at full Moon the valley shows the brilliancy characteristic of raised lunar surfaces, but the experienced eye can see that while above the level of the *Mare Imbrium* it is lower than the contiguous patches.§

* A peak in the vicinity is the highest part of the Moon ("Der Mond," B. & M.)

† Under favourable libration and latitude the exact locality of South Pole can be observed through an opening in *Malapert* (B. & M.)

‡ On this range rises a peak sufficiently high to be *always* enlightened by the Sun and so to enjoy perpetual day, while the plains on the north base experience as constantly the ever-recurring vicissitudes of day and twilight (B. & M.)

§ Positive and negative photographs were taken of the Moon during last

On the north-west of the valley the elevated patch appears dome-shaped, and as *Autolycus* occupies the central position, the idea is suggested that the Ringgebirg was the ultimate result of the upheaving forces. At the north-west point above *Hadley* there is (as in the lunar map) a slight depression.

The entire Apennine region (Das Apenninen Gebirg und Hochland of B. & M.) is a large triangular district of which the promontory north-west of *Hadley* is the apex and the north-east converging side the one arresting our attention. In this side may be noticed about seventeen marked breaks, with the principal elevations standing out in high relief. Gigantic spurs (Vorberge, B. & M.), and rather high ranges are also to be seen running south-west, leaving profound intervening valleys. Beginning north, *Hadley*, *Aratus*, *Bradley A*, *Bradley*, *Huygens*, *Huygens A*, *Wolf A*, can be traced towering up more or less strikingly,—*Huygens A*, the highest (about 21,000 feet, B. & M.)

The physical characters of this part of *Apenninus* are those of a mountainous region, abruptly terminating on one side, running for a great distance in a course very direct, with subordinate ranges branching off at right-angles to the coast-line. These features present a structure very similar to that displayed on good maps of the Himalayan chain, where the great spurs and the lesser elevated lines of hills, are at right-angles to its main axis, while the sudden fall of *Apenninus*' coast-line to the level of *Mare Imbrium*, is also analogous to the Asiatic break-down of the high land of Thibet to the low level of Hindostan.

It is as important to the selenologist as it is to the geologist to obtain some approximate knowledge of the chronological sequence of the different formations (or modifications) of the lunar crust. The general appearance of *Mare Imbrium*, running up direct (and to so great an extent unchanged in area level) to the very foot of *Apenninus*, would lead to the inference that their physical condition existed anterior to the more local and limited upheaval of the small *patches north and south* of the valley of *Archimedes*. The *north patch* indicates that expansive forces had first produced the domical form, and then ultimately found vent in the ejection and building up the crater of *Autolycus*. These are symptoms of continuous, or at least, of protracted igneous action. *Archimedes* is probably posterior in age to the *south patch*, because the great ridging and elevation of high hill ranges seem to be rather connected with his protrusion. Again while it is clear that the *Apennine* range ran up to and beyond *Eratosthenes*, it is also probable that it once extended to *Copernicus*. If so, it would then follow that the elevation of this most magnificent Ringgebirg with its radial system, would, *pro tanto*,

month, and at the full Moon of this. On cursory views both showed decided triangular shading, but the translucent glass positives (much magnified) viewed both on collodionized and non-collodionized surfaces, and under different angles of light (*over black velvet*) distinctly showed the details of the *Archimedean* valley and the *south-west patch* ridged with hill ranges.

have obliterated the continuity of this range (as appears to be the case), and consequently be newer in age. On tracing carefully the direction of one of the north rays of *Copernicus* we find it to bifurcate near the *south patch*, and one of the branches to touch and become lost in or under it. There are often great niceties in judging of the effects of these rays, and inferences must be drawn cautiously, but, on the whole, I think that this branch underlies without ridging the patch. The connection of the raised parts with the branching and curvilinear elevations being already hypothetically accounted for, it would then follow that the ray is older than the south patch. Lastly, as to the remaining question of the relative ages of the *Apennine* range and *Eratosthenes*, I should consider this Ringgebirg as last formed, because its circular shape gives evident proof that nothing has interfered with its form since its upheaval, while the *Apennine* range seems to have been clearly interrupted by *Eratosthenes*. Hence, I infer the relative ages of these localities and districts to stand thus in a descending series (beginning with the newest), *Autolycus*, *Archimedes* (and perhaps the hill ranges on the south patch), *north and south* patches, *Copernicus*, *Eratosthenes*, *Apennine* range, *Mare Imbrium*.

Ensleigh Observatory, Lansdowne, Bath,
27 Sept. 1869.

On his new Observatory at Churt, Surrey.

By R. C. Carrington, F.R.S.

I have bought the freehold of almost nineteen acres of land situated six miles to the south of Fareham, on a part of Frensham Common, in the village of Churt. It contains a conical hill, sixty feet high, which is entirely detached, and it was this that induced me to purchase it. It goes by the name of The Middle Devil's Jump, as in another spot near by there is the Devil's Punch-Bowl. Its situation by the Ordnance Survey is

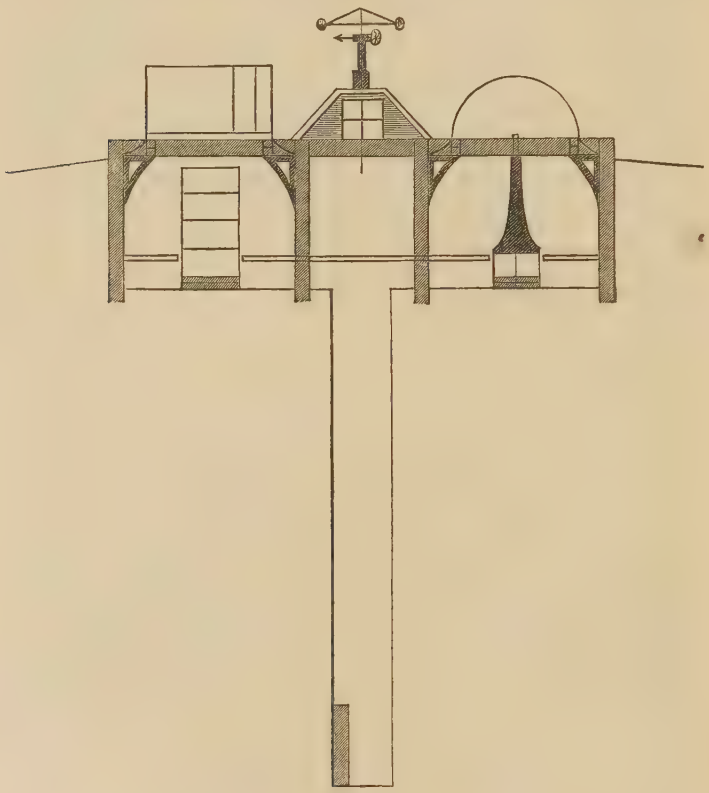
Lat. 51° 8′ 49″ N.
Long. $0^h\ 3^m\ 1^s{\cdot}7$ West of Greenwich.
Alt. about 340 feet above Liverpool.

Farnham is the nearest post-town and railway-station. It is necessary to state this, as I find letters and parcels continue to be addressed to me at Redhill, though I have left it seven years ago.

There are three things I wish to speak of, the Observatory in itself, the clock, and the principal instrument.

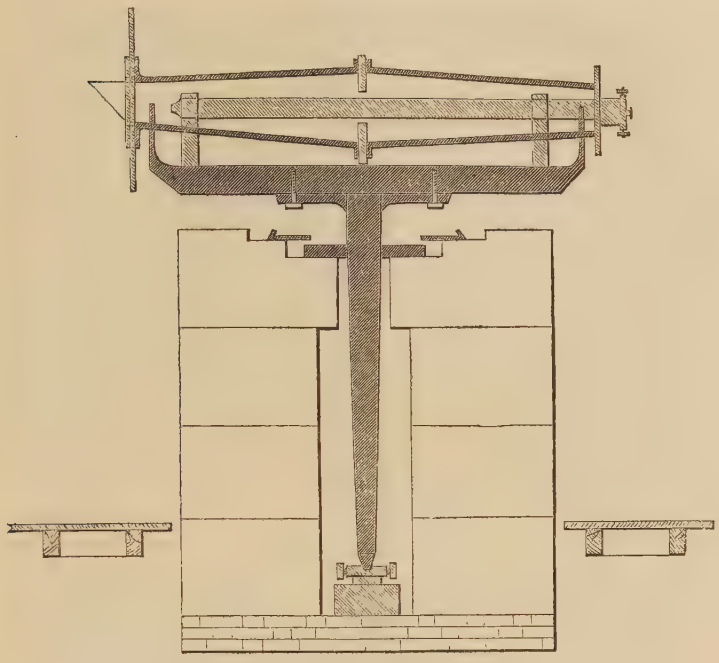
Being on a hill I did not want elevation, so I have sunk the

Observatory below ground, just peeping out over the soil. But I have further sunk a dry well, six feet in diameter, to the depth of forty feet from the centre of the Observatory, and with a horizontal shaft communicating with the south side of the hill, 166 feet in length, closed with three doorways. This is principally intended for the clock, for I am determined that one clock at least shall be properly mounted, at a position of invariable temperature and in an air-tight case. I propose to reduce the pressure to twenty-seven inches of mercury. My model clock is at present the one at Bidstone Observatory, made by the late

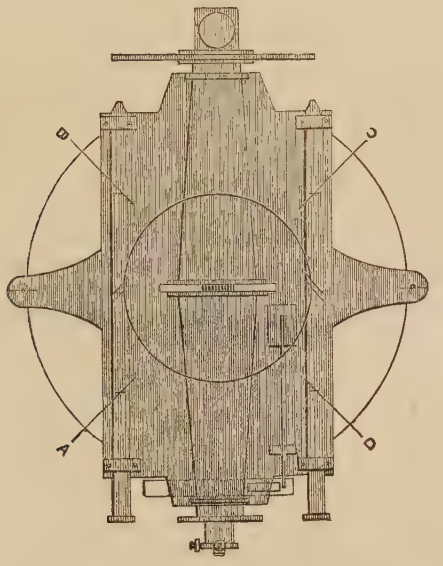


Observatory.

Mr. Richard Bond, with a gravity escapement, and its rates vary not more than $0^{s}.04$ per diem in November and December. Still Mr. Frodsham, to whom I applied, assured me that, provided I had a good pendulum, he did not care what the escapement was, and I have reluctantly gone back to the old dead beat. The communication with the moveable dial above is by galvanic



Altazimuth. Dark shade steel or cast-iron, light brass or gun-metal.



Altazimuth viewed from above. Scale $\frac{1}{10}$.

wires, and all difficulty about hearing is overcome. I hope I shall shortly have the most perfect clock in England, perhaps in the world.

Of the principal instrument I have to announce that I have decided on having an altazimuth, but constructed on a new principle. I have fixed on Steinheil's principle of making the horizontal axis the effective optical axis, by placing the object-glass at one end, and the eye-piece at the other, with a prism outside the object-glass. The casting of the prism took over three months, but Messrs. Chance succeeded at last ; six inches aperture is adopted, and six feet three inches focal length. It is some comfort to think that never need the telescope be raised, only turned round, and the observer always under cover. The principal vertical axis is steel, and the bed-plate of cast-iron. The telescope-tube is also cast-iron. The circles are both of thirty inches diameter; the altitude-circle of gun-metal solid throughout, that is without spokes, and cut out from a much larger piece. The microscopes are of five feet focal length, and I hope to try photography with them.

On certain important conclusions deducible from the Observations made on the Transit of Mercury at Greenwich, on November 5th, 1868. By Richard A. Proctor, B.A.

From a careful study of the investigations to which the observations made on the transit of *Venus* in 1769 have been subjected, and more especially of the masterly researches of Mr. Stone in the *Notices* for October 1868, I came long since to the conclusion that one of the chief points to be considered by astronomers in preparing for the coming transits is the effect which has been termed the "clinging of the limbs of the Sun and planet" near the time of true internal contact. In an article which appeared in the *Daily News* of November 4, 1868, I called special attention to the advantages which might result to science if observations were made on the internal contact of *Mercury* with reference to this effect. I wrote as follows: "Though transits of *Mercury* are not in themselves very important phenomena, it cannot be doubted that astronomers will avail themselves of the opportunity to practise, so to speak, for the approaching and far more important transits of *Venus* in 1874 and 1882. They will inquire whether the magnifying power of the telescope made use of has any bearing upon the duration of the deceptive appearance, or whether darkening glasses somewhat more powerful than those usually employed may not diminish the irradiation to which the phenomenon is due." A week later I wrote a letter to the same effect to the editor of the *Scientific Opinion*, and in a paper which appeared in that journal yet a week later (but was

in fact written at the same time as the letter), I called particular attention—first to the bearing of Mr. Stone's re-investigation of the transit-observations of 1769 on the subject of the coming transits ; and, secondly, to the probable value of a "well-concerted plan of observations or even of a comparison, *inter se*, of observations already made on the peculiarity in question."

I mention these facts to explain the interest I take in the valuable series of observations made at Greenwich on the transit of *Mercury* in 1868, and particularly in the examination to which Mr. Stone subjected those observations. Certain highly important consequences seemed to me so obviously to flow from Mr. Stone's paper on the subject in the *Monthly Notices* for November, 1868, that in all cases where in treating the coming transits of *Venus* these consequences have been in question, I have in effect taken them for granted, not wishing to waste space in pointing out what Mr. Stone had already, as I conceived, demonstrated.

But as I judge from a passage in Mr. Stone's last communication to the Society that he himself reads his observations differently than I had been disposed to do, I feel that the importance of the whole subject will justify me in inviting attention to the conclusions which may, as I conceive, be directly deduced from Mr. Stone's "Remarks and Suggestions arising from the observations of the Transit of *Mercury* across the Disc of the Sun." (*Monthly Notices* for November, 1868.)

I refer now principally to the remark in his last paper, that the errors of contact-observations, "arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres." I had so read Mr. Stone's earlier paper as to believe its main object was to *prevent* observers from falling into this sort of error; and I still, after renewed and careful study, not only of that paper, but of the whole subject, can form no other opinion than that the practical value of Mr. Stone's researches is altogether greater than he seems to believe. Instead of looking upon his remarks as merely suggesting a difficulty, I find in them the clear indication of a method of escape from that difficulty.

To save space, I will take only the extreme cases of difference referred to in Mr. Stone's last paper.

Mr. Lynn, observing with the north equatoreal and power about 170, saw a delicate filament form itself between *Mercury* and the Sun's limb at $21^h\ 0^m\ 0^s.7$ G.M.T. The filament was so fine that we may believe that Mr. Lynn really "caught a phase very near that of real internal contact."

Mr. H. Carpenter, using the east equatoreal and a power of 70, observed the sudden formation of a broad ligament between *Mercury* and the Sun at $21^h\ 0^m\ 11^s.1$ G.M.T.; and on Mr. Stone's requesting him to point out which of certain figures (see *Monthly Notices* for November 1868) "appeared to him to represent most

nearly the appearance of the planet at the time of his observation, he without hesitation pointed out fig. (3)," but the phase, Mr. Stone says, was "somewhere between (2) and (3)."

Now in fig. 2 of the above-named paper, the ligament is rather less than one-half the diameter of *Mercury* in width, in fig. 3 rather more than one-half. We may suppose that in reality it was as nearly as possible one-half.

Therefore, supposing we wished to determine the time of real internal contact from Mr. Carpenter's observation, we ought, I apprehend, to proceed as follows.

We may assume, first of all, that irradiation does not tend to make the cusps seem appreciably nearer together than they actually are, because on no other hypothesis can we account for their squareness of outline. It follows, then, that the chord joining the cusps at the time of Mr. Carpenter's observation was equal in length to about one-half the diameter of *Mercury*. Hence it subtended at *Mercury's* centre an angle of about 60°, and therefore *Mercury* had really crossed the Sun's limb (normally) by a distance equal to *Venus'* radius × vers. 30°, approximately.

Now in the transit of Nov. 1868, *Mercury* traversed an arc of 41°, from external contact to external contact (Sun's semi-diameter 16′ 10″·7, *Mercury's* 4″·9) in $3^h\ 38^m\ 18^s$. And it is easy to calculate from this *Mercury's* normal velocity, which I find was 0″·06412 per second. Hence the interval which had elapsed between real contact and the time when Mr. H. Carpenter observed the formation of the broad ligament, was

$$= \frac{4\cdot9 \text{ . vers } 30^\circ}{0\cdot06412} \text{ seconds of time,}$$

$$= 10\cdot3 \text{ seconds.}$$

Hence the calculated epoch of real internal contact is—

$$21^h\ 0^m\ 11\cdot1^s \text{ G.M.T.} - 10^s\cdot3$$

$$= 21^h\ 0^m\ 0^s\cdot8 \text{ G.M.T.}$$

Mr. Lynn's observation gives, for the same phase—

$$21^h\ 0^m\ 0^s\cdot7$$

Hence, when thus treated (and there is nothing forced in the above method, nothing, in fact, which is not strictly accordant with Mr. Stone's interpretation of the nature of the clinging), Mr. Carpenter's observation gives a result *differing by only one-tenth of a second from Mr. Lynn's.*

I do not suppose for a moment that the closeness of agreement in this particular case is not in part accidental. But it appears to show very clearly that we can, by treating in the same way observations made on *Venus* in 1874 and 1882, obtain results *much* more accurate than by neglecting the considerations flowing from Mr. Stone's remarks on the transit of *Mercury.*

Of course, if those who come to manipulate the observations of

1874 and 1882 should insist on assuming that a phase caught by one observer is identical with a different phase caught by another, the old cause of errors will creep in. But in doing so they would be blinding themselves to the real value and significance of the observations made at Greenwich on the morning of November 5, 1868. This will not happen I should imagine. If, however, an Encke of the future should make such a mistake, doubtless there will not be wanting a Stone of the future to correct the results so obtained.

So long as the observers in 1874 and 1882 indicate as closely as they can the apparent breadth of the dark ligament, it must always be possible to determine the moment of real contact much more exactly in the manner above indicated than by assuming the phase observed to be actually a contact. It is for this reason that I have been always unwilling to accept the view that the probable error will be proportional to the slowness of the planet's normal velocity. I believe, on the contrary, that, during a transit of slow normal velocity, the time given to the observer to form several estimates of the breadth of the ligament, will counterbalance (not wholly, but to an important extent) the absolutely greater time-intervals which separate different phases of the internal contact. I have not before entered at length into the reasons which led me to this view, because they seemed so clearly deducible from Mr. Stone's valuable remarks on the transit of *Mercury*.

In the preceding paragraphs I have referred only to ordinary eye-estimates of the breadth of the dark ligament. It seems so obviously suggested that the eye-piece of each telescope made use of should be provided with an arrangement for estimating the breadth of the ligament — as compared with the diameter of *Venus*, that perhaps it may seem unnecessary that I should mention the point. Still, as I have found hitherto no reference to the necessity of this being attended to, I may be excused for remarking on it.

Micrometrical measurement is, perhaps, not desirable, though it would clearly be possible to apply it in an effective manner. It is not necessary, however, because absolute dimensions are not required. All we want is to know the ratio which the breadth of the ligament bears to the diameter of *Venus*. Hence if we adopt any of the numerous contrivances by which a series of cross-lines (or two series at right angles to each other would be even better) can be made to appear in the field of view with *Venus*, it will be the simplest possible matter for the observer to determine the relative breadth of the ligament — and this, even though at the moment of contact neither set of cross-lines should be absolutely normal to the Sun's limb at the place of contact.

If it should be thought advisable (as was suggested in November 1868 by the Astronomer Royal) that places of observation should be so selected that *Venus* will cross the Sun's limb either near its highest or near its lowest point, all that is requisite is,

that the place of observation should be taken as near as possible to the curved diametral arrow which lies across my two charts of the Earth in the *Monthly Notices* for June 1869 (plates 5 and 6). Most of the stations already dealt with are little affected by this condition; but, for observing retarded egress, all the Indian stations will be found (in this respect) far better than Alexandria. At Peshawur, for instance (a place already superior as respects coefficient of parallax and solar elevation), *Venus* will leave the Sun almost exactly at his uppermost point, whereas at Alexandria the point of last contact will be about 34° from the uppermost point of the Sun's limb.

A New Theory of the Milky Way. By R. A. Proctor, B.A.

Sir W. Herschel's respect for existing analogies—a quality which is perhaps of all others the safest guide for the scientific explorer,—led him to adopt as the means of interpreting his noble series of star-gaugings the hypothesis that there is a general uniformity in the distribution of the stars through space. He adopted this hypothesis not from a conviction of its being actually true, nor even from the belief that it is approximately so, but simply because existing analogies seemed to render it probable, and because it formed a convenient basis for calculation. The existing analogies were those presented by the solar system. In this system, Sir W. Herschel recognised a number of discrete bodies, not equal indeed, but comparable *inter se* in magnitude; not uniformly distributed, but still not aggregated towards one or another part of the solar scheme. And making such modifications as seemed requisite in comparing a system not regulated by a vast central orb with a scheme like our solar system, it seemed likely to him that a general equality of magnitude, and a general uniformity of distribution, might be found to prevail among the members of the sidereal system.

We now know that the ideas which astronomers had formed of the solar system in Sir W. Herschel's day were very far indeed from being correct. We see in the solar system a complexity of detail, and a variety of form, structure, aggregation, and motion, which were altogether unknown a century ago. And I cannot doubt that if the view we have of the solar system had been presented to Sir W. Herschel, he would have adopted as the basis of his star-gaugings an hypothesis differing altogether from that of which he actually availed himself. He would have argued, that as, in the solar system, there are bodies like the planets, far surpassing the other members of the scheme in magnitude and in importance; as it contains zones of minute bodies, such as the asteroids and the satellites composing the rings of *Saturn*; myriads of meteoric systems, and countless thousands of cometic systems, so doubtless in the sidereal system there are many forms of matter. If the analogy of the solar system is to be our guide, we

must look for suns equalling or surpassing our own in magnitude and splendour ; for clusters and systems of minor suns, whose united mass may fall short of the mass of one of the primary stars; for aggregations of matter in portions relatively so minute as not even to be comparable with the small stars found in true star-clusters ; and finally, for systems composed of materials, or at least of forms of matter, differing as widely from the substance of the suns, as the matter composing a comet does from the substance of the Earth or of *Jupiter*.

But, even independently of analogies such as these, his own series of observations led Sir W. Herschel to feel more and more doubt, as he proceeded, respecting the hypothesis which he had made the basis of his calculations. It is only necessary to compare the later papers he sent in to the Royal Society with the earlier ones, to find that views altogether inconsistent with his initial hypothesis were opening out before him. It was in those later papers that he spoke of star-groups in the Milky Way clustering towards opposite regions of the heavens ; of stars arranging themselves into separate systems ; and of the signs which the heavens present of the action of processes of aggregation, causing "the gradual dissolution of the Milky Way."

Sir John Herschel, also, in carrying out the system of star-gauging among the Southern stars, was led to notice many features which the hypothesis of a tolerably uniform distribution of stars could not satisfactorily explain.

Judged according to Sir W. Herschel's fundamental hypothesis, the sidereal system came to be regarded as forming a figure resembling that of a *cloven disc*, and the Milky Way was explained as being due simply to the greater extension of the system in the direction of the medial plane of this disc. Sir John Herschel, however, from his observations of the Southern heavens, was led to suspect that this theory was not strictly correct. He speaks in one place of certain evidence, according to which the Milky Way would come to be regarded as a flat ring seen edge-wise. And in many places he speaks of the difficulty of understanding certain features according to the views usually accepted.

It seems to me that the evidence collected respecting the Milky Way is sufficient to lead us to quite another view of its structure than that to which Sir W. Herschel was led by an hypothesis founded on the incomplete theories which astronomers in his day had formed respecting the solar system.

Let us regard the matter altogether independently of preconceived opinions, and judge simply as the evidence may seem to teach us.

In the woodcut, the outer figure represents the Milky Way according to the drawings and description of Sir John Herschel. The mode in which it is delineated needs no explanation.

Now in regarding this picture of the Milky Way, we are forced, I think, to the conclusion that neither the cloven-disc theory, nor the flat-ring theory, accounts satisfactorily even

for the principal features of the Milky Way. For example, the great gap which crosses it in *Argo*, nearly in the widest part of the single branch, seems utterly inexplicable on either theory. There is no way of accounting for that gap if we are really supposed to view the Milky Way from a point within its figure, and that figure resembles—however roughly—either a cloven disc or a flat ring.

But let us pass to other features. Travelling towards the right from the gap, we come to the strange semicircular cavity

with a well-defined outline, which Sir John Herschel has described in such striking terms. A cavity of that figure is a remarkable phenomenon, and is surely inexplicable either on the flat-ring or cloven-disc theory. But the mere distinctness of the outline is one of the strongest possible proofs that the stars which form that portion of the Milky Way constitute a distinct clustering aggregation from which we are separated by an enormous and comparatively star-less interval.

We come next to the Great Coal-sack near *Crux*, almost opposite to which is a well-marked opening in *Cygnus*. There are

also other strange openings through different parts of the Milky Way.

Now I cannot but think that an argument similar to that which Sir John Herschel has applied with so much force to the Magellanic clouds applies to the openings in *Crux* and *Cygnus*. He argues that because the Magellanic clouds approach roughly to the circular figure, therefore, in all probability, their real figure is that of a sphere: the chance is small that one of them is a cylindrical system seen endwise, but the chance that both are is altogether evanescent. Now applying the same principle to the Coal-sacks, we are led with equal certainty to the conclusion that these apertures are not cylindrical or tunnel-shaped openings seen endwise, but *if they are really openings at all* they are openings through a system which is not very much deeper—measured in the direction of the line of sight—than the greatest width of the aperture itself.

Judged in this way, the parts of the Milky Way which lie round a "Coal-sack" would have a roughly circular section, and not that enormous extension in the direction of the line of light, which has been assigned to them.

I cannot see that this argument is at all less sound or less effective than that which has been applied by Sir John Herschel to the Magellanic clouds.

There is another feature referred to, and I believe discovered, by Sir John Herschel, which is also full of meaning. I refer to the existence of narrow and sometimes convoluted streams of stars, branching out from the Milky Way itself. Sir John Herschel says of these that we ought to look on them as in all probability planes or scrolls of stars seen tangentially, and not as branch-shaped extensions bristling up from the general level of the Milky Way. And undoubtedly if the Milky Way really have a great extension in the direction of the line of sight, it is just that we should so regard these outlying streams. But if we judge of them without any reference at all to pre-existing theories, we are guided by strong arguments from probability to form a very different view. The chance that a plane system of stars, and still more a scroll of stars, should be turned so directly towards the Sun as to present to us the appearance of a straight or convoluted line or narrow stream of stars, is small indeed. The probability that several should be so situated may be regarded as evanescent.

Accepting these streams as having a roughly circular section, we are led to the conclusion that the Milky Way from which they extend has a similar section. In fact, as Sir John Herschel held these streams to be really planes or scrolls because (I assume) he assigned to the Milky Way a great lateral extension, so by inverting this argument I am led to believe that the Milky Way has not a great lateral extension (compared I mean with its thickness), because the streams extending from it have in all probability a section of roughly circular figure.

Other arguments there are that space will not permit me to dwell upon here, which point in the same direction.

Now, of course, if the Milky Way forms in reality a stream of stars *amidst the sidereal system*, the appearance which it presents upon the heavens might be expected to afford some information as to the shape of that stream, or at least of that portion which is cognisable by us. It must be admitted, however, that the problem of interpreting this wonderful stream is one of enormous difficulty. Perhaps it is one which men will never be able to accomplish in a perfectly satisfactory manner. It is only necessary to contemplate that marvellous maze of star-streams around *Scorpio* and its neighbourhood, and to read the account which Sir John Herschel gives (in his *Cape Observations*) of the telescopic aspect of this region, to feel how far we are at present from being able rightly to interpret the mysteries of the Galactic circle.

The bolder and more striking features of that circle may, however, be studied with a better hope of their being successfully interpreted. A theory which will explain the gap in *Argo*, the wide break of one stream in *Ophiuchus*, the varying brightness of the principal stream in different parts of its length, and other features of this kind, may reasonably be sought for.

I have endeavoured in the inner circle of the figure to indicate a spiral which seems to me to account for the most striking features of the Milky Way.

Following that spiral round from the part where the two loops approach each other, we have the following relations :

First of all, the gap is explained by the fact that the two loops do not meet. Then, remembering that the spiral is supposed not to lie in one plane, but (as the contorted figure of the Milky Way obviously suggests) to have been swayed out of that plane by varying attractive influences, we see that where the line of sight is directed tangentially to either loop, the Milky Way might be expected to have greater width than elsewhere. This explains the fan-shaped expansions on each side of the gap. Then on each side of these expansions we see the Milky Way double, which obviously corresponds to the relations exhibited by the two loops. Following the wider loop, we see that the double part of the Milky Way on this side extends nearly through a complete semicircular arc. The Coal-sack is explicable as due to the apparent intercrossing of the two contorted streams which really are at different distances from the eye.* The break in the further branch seems readily explicable as due to the great distance of a portion of this branch. But here the theory derives a singular support from the actual relative brilliancy of different parts of the Milky Way in this neighbourhood. Every astronomer knows how strangely the light of the Milky Way varies in and near

* In the large maps of the S.D.U.K. the Milky Way is depicted near *Crux* and *Argo*, as if the object of the draughtsman had been to support my theory. In Sir John Herschel's drawing, however, there are no such varieties of brilliancy.

Cygnus. The branch which extends from the Northern Coal-sack towards *Albireo* is at first far the brightest, and then fades off so much that in *Ophiuchus* it is wholly lost. The other branch, on the contrary, gradually increases in brightness, until in *Aquila*, and, further on, in *Sagittarius*, it forms the brightest part of the whole Milky Way. Now this part which is so very bright, corresponds to the part which my spiral brings so very near to the Sun.

Passing on to the termination of the second branch near *Cygnus*, it will be noticed how the spiral explains the strange extension of milky light from *Cepheus* towards the north pole.

Thence the stream is single, growing gradually fainter with increase of distance towards *Canis Minor* and *Monoceros*.

The spiral I have depicted seems so satisfactorily to account for several of the more striking features of the Milky Way, as to suggest the idea that it probably corresponds somewhat closely to the real figure of that star-stream. I am sensible, however, that many peculiarities remain unexplained by, though they are by no means opposed to, my theory. It must be remembered that any objections founded on a presumed equality of stars throughout the Milky Way, or of a general uniformity of distribution throughout the spiral stream, do not require to be met; because at the very beginning of this inquiry I have abandoned such hypotheses as inconsistent with existing analogies.

For example, there may be parts of this Milky Way so constituted, that if we were to remove further and further from them, we should see them gradually assuming the form of irresolvable nebulosity. But there may be other parts which would never assume that appearance, let their distance be what it might — the distribution and magnitude of the component stars being such that the stars would vanish through effect of distance, before the distances apparently separating them became evanescent.

I may add as a striking confirmation of a portion of these views, that among the lucid stars along the part of the Milky Way which lies nearest to the Sun, according to my view, are those which have been actually found to be nearest to us.

It must be understood that I regard the Milky Way as simply the condensed part of a spiral of small stars, which has been swayed into its present figure by the influence of large stars — the lucid stars seen in the Milky Way. The myriads of small stars not lying in or near the Milky Way, must yet belong to the same system, and in some instances seem to obey somewhat similar laws of aggregation. The nebulæ, so far as the evidence from probability extends, would appear to be groups formed from among those stars that have not fallen under the influence of the large stars which have brought the Milky Way spiral to its present figure. In the Magellanic clouds, we see the action of processes which have tended to form spherical clusters of enormous dimensions, in which both forms of aggregation are met with.

Why, in different parts of the sidereal system different pro-

cesses of aggregation should have taken place, we cannot yet distinctly see. But some of the striking discoveries which have recently been made by astronomers afford promise that light will soon be thrown on these perplexing questions.

P.S.—If my views respecting the Milky Way are correct, it obviously follows that there are parts of the Milky Way where traces of annual parallactic displacement might be looked for amongst *telescopic stars*. One instance of such motion would force us to modify all the views at present accepted respecting the sidereal system.

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Printed by STRANGEWAYS and WALDEN, Castle St. Leicester Sq. and Published at the Apartments of the Society, Jan. 10, 1870.